says (p. 60) that he "was informed" (which implies that he did not previously know) that "one of Van der Waals' papers . . . contains an elaborate study of the molecular pressure in fluids."

Again he says, "I have left the passages . . . which refer to this subject in the form in which they stood before I became acquainted with Van der Waals' work. I have not sufficiently studied his memoir to be able as yet to form a definite opinion whether the difficulty . . . which is raised in Appendix E. can, or cannot, be satisfactorily met by Van der Waals' methods."

Further, he states that he "had been under the impression . . . that Laplace's views had gone entirely out of fashion—having made, perhaps, their final appearance . . . about 1850."

As a matter of fact, Van der Waals adopted Laplace's views in 1873, and his formula differs from the expression $pv = RT$, only by the introduction of two terms, one of which is obviously an additional pressure such as is deduced from Laplace's theory.

I do not think that any reader could be expected to conclude from these passages in Prof. Tait's Addendum that when writing the paper he had long known the "main features of Van der Waals' investigation." To me they seemed to mean that he had not previously been acquainted with Van der Waals' work, nor with his methods, nor with the facts that he studied molecular pressure and adopted Laplace's ideas.

While, therefore, I willingly submit to Prof. Tait's correction of the phrase that he had "never heard of Van der Waals," I cannot admit that, on the evidence then before me, I did him any substantial injustice.

(2) I very much doubt whether the distinction between the ultimate volume and the molecular volume can be maintained if the equations are treated as empirical; and even if they are not, I doubt whether the ultimate volume, as defined by Prof. Tait, has any real physical meaning. The value of v when $p = \infty$ is independent of the temperature, whether deduced from the theoretical formula to which Prof. Tait refers (p. 48), or from those of Van der Waals or Clausius: hence it must (from this point of view) be the molecular volume. In the case of Prof. Tait's new equation, which was published after his Report was completed, and which is the only one I had not seen when I wrote the review, the results when we put $p = \infty$ or $T = 0$, are such as to show that its application to these extreme cases is not legitimate. My own view is that such algebraical solutions are worth very little, and I only discuss them because I wish to show that if we admit them at all they justify my treating Prof. Tait's number as an estimate of the molecular volume.

(3) I cannot say that I think that Prof. Tait's reason is adequate. The Royal Naval College at Greenwich has done more for our naval officers than he would have us believe, and, if it were not so, the *Challenger* Reports are not addressed to members of any one profession, nor intended for English-speaking scientific men alone. Their cosmopolitan character is shown by the fact that bound up in the same volume with Prof. Tait's Report is another by a distinguished Belgian geologist.

Foreigners have helped to describe the specimens which our Expedition collected; they will read the Reports which our experts have written. It would have required but a few minutes' work, and a few additional lines of print, to have given the final results in terms which they would have understood at a glance.

(4) The analogy is fallacious. Prof. Tait has devised a formula into which he introduces two quantities (age and speed), which are commonly expressed with reference to different units of time.

I pointed out that he had expressed in the same formula (contrary to common usage) the same quantity (pressure) in terms of two different units, of which one is not ordinarily used by many of those who will make use of his work.

As to the last paragraph, I have only two remarks to make, First, that I think Prof. Tait does himself injustice in regarding a description of apparatus devised by another, and the discovery of a blunder of the Bureau International, as two of the most important things in his Report. Secondly, that I think the imputation of motives should be banished from scientific discussions.

In conclusion, I wish to add that probably I should have left Prof. Tait's defence unanswered if he had not accused me of unfairness. I have no desire for any controversy, and no wish to impugn his knowledge of the theory of gases. But he will forgive my reminding him of the old saying, "*Noblesse oblige.*" A classical research should not be published in a state which leads the reader to the conclusion that the author was only just becoming acquainted with facts which bear upon his work and have been long before the world. As a reviewer, I formed the opinion that the Report under discussion was open to this criticism. As a reviewer, it was my duty to express my opinion in all honesty, and, as I hope, in all courtesy.

ARTHUR W. RÜCKER.

Visualized Images produced by Music.

IN the annexed paper, and in her own words, are related the very curious effects produced on a lady friend by certain musical tones and orchestral combinations. They are so very singular, so entirely outside my experience, and, withal, so inexplicable, that I shall be glad if you will give them a place in your columns, in the hope that some of your readers—physiological or psychological—may be able to throw some light on them.

I should state that the lady is in perfect health, is very intelligent, an accomplished musician, and not at all, in this or any sense, the victim of a disordered imagination. She is quite conscious that these spectral images have only a subjective existence, though visually they have all the vividness of presentment which belongs to realities.

At the first blush it would seem as though these apparitions were in some way a response to stimuli sent through the auditory nerve; but this, if any, is an imperfect explanation, since it will be noticed that occasionally these visualized pictures *slightly precede the instrument they belong to.*

This fact suggests that a state of unconscious expectancy may be a factor in their reproduction, but it fails entirely, I think, to account for their initial appearance. GEO. E. NEWTON.

25 Woodland Road, Gipsy Hill, S.E.

"The sound of an oboe brings before me a white pyramid or obelisk, running into a sharp point; the point becoming more acute if the note is acute, blunter if it is grave. The obelisk appears to be sharply defined and solid if the note is loud, and vague and vaporous if it is faint. All the notes of the 'cello, the high notes of the bassoon, trumpet, and trombone, and the low notes of the clarionet and viola, make me see a flat undulating ribbon of strong white fibres.

"The tone of the horn brings before me a succession of white circles of regularly gradated sizes, overlapping one another. These circles and the ribbon float past me horizontally, but the point of the obelisk seems to come at me.

"In an orchestra, when the violins strike up, after the wind band has been prominent for a time, I see often, but not always, a shower of bright white dust or sand, very crisp and glittering. I am taking note of the recurrence of this impression, and think it is becoming more frequent, but it is not invariable like the others.

"I have heard a great deal of orchestral music all my life, but I have only noticed these effects for four or five years. They gained gradually in frequency and clearness, and now the first three are invariable.

"If I know the scoring of a piece well, the various effects *slightly precede* the instrument they belong to; only the objects are vague and faint till the sound begins.

"Sometimes, if an oboe passage has an intense and yearning character, the white point comes so near me, and moves so rapidly, that I think it *must wound me.*

"I am very anxious to make it clear that I am not trying to describe a mental state by symbols, but that *I actually see* the point, the fibres, and the circles. Generally they seem to float half-way between me and the orchestra.

"If only one class of instruments is used, the effect does not extend beyond the opening bars: for instance, in a string quartette I only see the white sand for a moment at the beginning; if, however, wind and stringed instruments are combined, I see the various effects again and again in one piece."

Foreign Substances attached to Crabs.

IN your issue of December 26, 1889 (p. 176), Mr. Pascoe drew attention to the cases of certain crabs which are frequently found covered with sponges, algæ, shells, &c., and brought forward also the well-known case of the Gastropod *Phorus.* He at the same time confessed that he could not see "where protection came in" in any of the cases which he cited. Mr. A. O. Walker, on the other hand (NATURE, January 30, p. 296), regards it as obvious that the attachment of these foreign substances is a useful adaptation for purposes of concealment. Prof. Herdman also (NATURE, February 13, p. 344) bears witness to the

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"scarcely recognizable" appearance of the crab *Hyas* when covered with algæ, &c. Indeed, no one who has seen one of these crabs brought up with the dredge, or has found a well-covered *Stenorhynchus* on our own shores, can seriously doubt the usefulness of the habit in rendering the animal inconspicuous. In *Stenorhynchus* and *Inachus* the process of "dressing" with weeds and zoophytes has been described by Bateson (Journ. Mar. Biol. Association, vol. i. 1889, p. 213), and it is seen from his description that, as also in the cases of *Dorippe*, *Pagurus*, *Dromia vulgaris*, &c., the foreign substances or animals become attached to the body not by accident but by the act of the crabs themselves.

Now Mr. Walker, in regarding *all* these cases as instances of adaptation for concealment, has overlooked the fact that in two of our British species of hermit crab (*Pagurus bernhardus* and *P. prideauxii*) it is the habit of the animals to prefer, and often to fight for, shells which are rendered conspicuous by the attachment to them of species of Anemone, in the one case *Adamsia rondeletii* (*Sagartia parasitica*), in the other *Adamsia palliata*. Another British species (*Pagurus cuanensis*) is almost invariably found inhabiting a shell enveloped in the sponge *Suberites domuncula*, which is frequently of a conspicuous orange-red colour Only in the smallest species of *Pagurus* (*e.g.*, *P. lævis*) does the animal depend invariably upon an inconspicuous appearance for its safety.

The value to the crabs of a preference for shells to which Actinians are attached is found in the fact that these gaily-coloured animals are carefully shunned by fishes on account of their stinging powers; and although hermit crabs themselves are very palatable to fishes, their association with Actinians, while rendering them conspicuous as they move about, is at the same time an efficient protection from the persecution of their enemies.

This also explains the habits of the two Mauritian crabs, which, according to Möbius, carry about a sea-anemone in each claw.

The sponge with which *Pagurus cuanensis* is associated is (like all other sponges with which I have experimented) exceedingly obnoxious to fishes on account of its bad smell and taste. I have never succeeded in inducing a fish of any species to swallow a fragment of the sponge; but on the contrary the smell is in most cases quite sufficient to drive the fish away. The association with the sponge is therefore here also an efficient protection, for I know of no fish capable of extracting the crab from its retreat. It is seen from this that the case of *Dromia vulgaris* should probably be removed from the category of adaptations for concealment, and, like the cases of *P. bernhardus*, &c., be included in a special group of *warning* adaptations.

There yet remains the interesting case, adduced by Dr. R. von Lendenfeld, of *Dromia excavata* associated with a Compound Ascidian of the genus *Atopogaster* (Herdman). This, I believe, will be found to belong to the same category of warning adaptations, for after repeated experiments with Compound and other *Tunicata* at the Plymouth Laboratory I can state that these animals are essentially inedible to fishes. The inedibility is in large part due, as in the case of sponges, to the characteristic odour which *Tunicata*, and more especially Compound *Tunicata*, give out, and in no family (excepting perhaps the *Botryllidæ*) is this better marked than in the *Polyclinidæ*, the group to which *Atopogaster* belongs. Bearing in mind also the fact that Composite Ascidians frequently vie with sponges and Actinians in the possession of varied and conspicuous colours, it is rendered practically certain that the case of *Dromia excavata* is another instance of this same type of adventitious warning contrivances.

Thus the edible (the edibility is not yet proved for foreign species) *Crustacea* which attach foreign substances to their bodies may be divided into two groups:—

(α) Those which are rendered inconspicuous in relation to their natural surroundings by the habit; *e.g.*, *Stenorhynchus*, *Hyas*, *Dorippe*, *Pagurus lævis*, and young forms of *Pagurus bernhardus*, &c.

(β) Those which associate themselves with animals, easily recognizable by, and possessing qualities offensive to, their chief enemies; *e.g.*, *Dromia vulgaris* and *excavata*, *Pagurus bernhardus*, *prideauxii*, and *cuanensis*. WALTER GARSTANG.

Laboratory of the Marine Biological Association,
Plymouth, February 21.

P.S.—From facts which Mr. Weldon and Mr. Harmer have communicated to me, it would appear that *Dromia vulgaris* frequently attaches Compound Ascidians (*Leptoclinum maculosum*, *Botrylloides Gasconiæ*) to its back instead of sponges, a variation of habit which is very interesting in connection with the apparently fixed habit of the Australian species.—W. G.

A Key to the Royal Society Catalogue.

"A CATALOGUER" appears to have misunderstood me in two points. In the index that I propose, the heads would not be numbered. Again, in forming an estimate of the size of the work, I made the supposition that the 8 papers of an author could be grouped, not under 8, but under 3 heads.

JAMES C. MCCONNEL.

A Meteor.

LAST night (Monday, the 3rd), as I was crossing the Old Deer Park to Richmond, I witnessed the flight of an exceptionally fine meteor, which shone out with great brilliancy notwithstanding the presence of a bright moon, which was almost at the full.

It appeared to start from the constellation of Leo, and travelled across the sky to the westward, vanishing some 10° or 15° above the horizon.

The night was very quiet at the time, and I heard no report.

T. W. BAKER.

Kew Observatory, Richmond, Surrey, March 4.

THE DISCOVERY OF COAL NEAR DOVER.

THE question of the existence of coal under the newer rocks of Southern England, which has engaged the attention of some of our leading geologists since the year 1855, has found its final answer in the discovery announced last week in the daily press. The story of the discovery is a striking example of the progress of a scientific idea, passing through various phases, and growing more clearly defined through opposition and failure, until ultimately it has been proved to be true, and likely to lead to industrial changes of national importance.

The question was originally started 35 years ago by Mr. Godwin-Austen in a memorable paper brought before the Geological Society of London, in which it was argued, from the character and arrangement of the coal-fields and associated rocks of Somersetshire and South Wales on the west, and of the Belgian and North French coal-fields on the east, that similar coal-fields lie buried beneath the newer strata of the intervening regions. Mr. Godwin-Austen pointed out that the general direction of the exposed coal-fields was ruled by a series of great east and west folds, running parallel to the great line of disturbance—"the axis of Artois,"—from the south of Ireland, through South Wales and Northern Somerset on the west, eastwards through Belgium and Northern France, into the valley of the Rhine, near Düsseldorf. Throughout this area the exposed coal-fields lie in long east and west troughs. This series of folded Carboniferous and older rocks formed also an east and west ridge along the line of the axis of Artois, which gradually sank beneath the waves of the Triassic, Liassic, Oolitic, and Cretaceous seas. Against this the strata of the three first of these rocks gradually thin off, while the coal-measures and other rocks of the ridge have repeatedly been struck in France and Belgium, and are now being worked immediately underneath the Cretaceous strata over a wide area.

The axis of Artois also, where it is concealed by the newer rocks in the south of England, is marked from Somerset eastwards by the anticlinal of the chalk of North Wiltshire, and the line of the North Downs, the general law seeming to be "that when any great folding and dislocation of the earth's crust has taken place, each subsequent disturbance follows the very same lines, and that simply because they are lines of least resistance."

Mr. Godwin-Austen, by combining all these observations, finally concluded that there were coal-fields beneath the Oolitic and Cretaceous rocks of the south of England,

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